

## AN UNPUBLISHED INTERVIEW WITH B. F. SKINNER

UNA ENTREVISTA NO PUBLICADA CON B. F. SKINNER

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As part of a research project on the experimental analysis of scientific behavior (Ribes, 1994), I conducted an interview with B. F. Skinner in 1990. Below I have transcribed the interview, which was taped as B. F. Skinner responded to a list of questions that I had previously mailed to him. This method prevented the possibility of any further interaction regarding his answers. The questions deal with his retrospective opinions about his scientific career and contributions. In spite of the fact that Skinner was interviewed several times and that he published several papers dealing with his scientific career (Skinner, 1956, 1979), the content of this interview provides some direct answers concerning the significance of scientific problems and concepts. I especially would like to emphasize the answers given to questions 2, 4, 7, 9, 10, 12 and 15, which deal with Skinner's opinions about theory, the nature of data, the similarity of verbal and non-verbal behavior, private events, and the contributions of John Watson and Gilbert Ryle.

The questions were organized into three groups. One group of questions dealt with general theoretical and methodological issues fundamental to an operant-conditioning approach to psychology. These questions explored Skinner's opinions about the concepts of reflex and contingency, the place of private events, the role of mathematization in behavior theory, and so on. A second group of questions were related to his personal research career. The third and final group of questions explored Skinner's retrospective views of his personal contributions and findings, such as the superstition experiment (Skinner, 1948). References to the papers alluded to in his answers appear at the end of the interview.

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**INTERVIEW WITH B. F. SKINNER** (January 25, 1990)

1) Why did you choose the reflex model -radically modified- to formulate a taxonomy and methodology fundamental for the development of a theory about behavior?

Skinner: I became interested in reflexes after reading books by Pavlov, Sherrington and Magnus. It was not good *preparation* for my research. Reflexes are concerned with the responses of organs. My research led me toward the variation and selection of the behavior of the organism as a whole.

2) In your writings you assume behavior to be orderly -as any natural phenomenon studied by positive science. Do you think this assumption influenced your conception about theory as data-language and of behavior research as technological control? Do you think that data are independent of theory or on the contrary that they are theoretically determined? [The writings alluded to are: Skinner, 1931, 1935, 1956].

Skinner: I did assume that behavior was orderly, and that basic assumption was no doubt important to me at all stages in my career. I do not regard it as an essential assumption, however. I think data are independent of theory although theories determine the selection of data. That is one of the things I have against theories. All data should be considered.

3) How do you conceive your analysis of behavior: As a molar or a molecular analysis? The selection of a representative property of the behavior stream leaves open both interpretations.[Interviewer's note: As a point of reference, Logan (1960) argued that the molar vs. molecular distinction is related to the aggregation rule concerning qualitatively different responses. The degree of restrictions in defining a response quality sets the boundaries between a molar and a molecular class. The larger the number of explicitly, qualitatively different responses in a class, the more molar the definition of a response class. On the other hand, the highest possible restriction consists of specifying a single quality of responding, which makes the definition of that response class a molecular one. The same criterion may be applied to analyze the macro-micro distinction as an aggregation of quantitatively different behaviors.]

Skinner: The distinction between molar and molecular has never been important to me. It is used in too many different ways. If it means the difference between how the organism works as eventually to be revealed by physiology (molecular) and why the organism works that way and why changes from moment to moment during the lifetime of the individual (molar), then I am on the side of molar. I have no interest whatsoever in how the organism works.

4) Why did you *conceptually* consider that behavior morphology -and its associated parameters such as duration, intensity (effort), geography and topography- were not important as compared with an effect of behavior -the closure of a micro switch? Is not this assumption in contradiction with your approach to verbal behavior?

Skinner: Operant behavior is primarily a matter of variation and selection. It is selected by a change in the environment, and behavior is most easily analyzed when that change is conspicuous, as it is in the closure of a micro switch. In daily life, actions working on the environment, serve the same function, and it is particularly true in the field of verbal behavior where it is the effect on the listener that defines the verbal operant.

5) Does the concept of *contingency* seem to you intrinsic to conditioning as a necessary and/or sufficient relation between events, or do you consider it as a mere temporal relation in regard to its functional properties?

Skinner: I used the word 'contingency of reinforcement' to represent three features of a situation: stimulation necessary for reinforcement; the behavior reinforced, and the reinforcing consequence. The temporal relations among these three terms are, of course, very important.

6) Which do you think are the critical experiments in your research career? Could you comment about the reasons you consider for each of them?

Skinner: In historical order, I think that the recording of a satiation curve in a cumulative record was important. It made visible an orderly change in the probability of behaving. Equally important was the proof that a single reinforcement made an observable change in the probability of pressing a lever. That followed because I was using a mechanical device to deliver food and the device made a noise which, in the procedure that I adopted, had chance to become a conditioned reinforcer and to occur instantly. I think that the demonstration that an absolute immediate reinforcer is so powerful was important. Another result was the demonstration of stimulus control over operant responding. The development of a discrimination as the extinction of the behavior in the presence of a stimulus not correlated with the reinforcement was one of them. Another was the peculiar behavior that I got when I was able to set up a discrimination in which the rat never responded to the unfavorable stimulus. Various schedules of reinforcement were, I think, important, as were the experiments in which responses to two levers were shown to interact in various ways under differential contingencies of reinforcement. Other points are, I think, covered in what follows.

[Interviewer's note: The earlier studies mentioned in this paragraph are quoted in Skinner 1938, and the work on reinforcement schedules is extensively

described in Ferster & Skinner, 1957].

7) Do you recognize in any of your experiments anomalies or contradictions with the fundamental findings of your research or the fundamental concepts of your theory?

Skinner: Many puzzling things have certainly turned up in my research and many questions have not yet been answered, but I do not regard them as contradictions, especially because I have never been very much interested in theory.

8) What do you think, in this context, about your classical experiment on superstition in the pigeon? Do you think that the concept of reinforcement is to be applied such as it was defined in regard to discrete, repetitive and predetermined responses? [Skinner, 1948].

Skinner: I repeated the superstition demonstration many times, often as a demonstration when lecturing to a group. Once you condition a reinforcer so that it acts instantly, then accidental contingencies are necessarily effective. And I have seen pigeons doing many other things superstitiously in addition to those reported in my paper. The things done I would not describe as discrete, repetitive, or predetermined.

9) Is not the emphasis on contingencies of reinforcement as a fundamental causal factor in contradiction with its dispositional logic functions -such as Gilbert Ryle defines it- in so much as it affects response tendencies? [Ryle, 1949].

Skinner: Philosophers have talked about dispositional functions and intention, but I am not particularly interested because an operant is a probability of response, not the response itself, and it is explained by past instances of reinforcement, not by any purposive or imagined future consequence.

10) To what extent do you think that reinforcement schedules and related parameters and measures constitute a beginning point in order to analyze human behavior? Do correlative concepts obtained in the research with nonsocial simpler organisms lose their meaning and usefulness when they are *extrapolated*?

Skinner: I believe human beings are susceptible to the variation and selection represented by operant conditioning, as they are susceptible to those represented by natural selection. But I do not think much research can be done on basic properties of operant behavior with human subjects if they've acquired a verbal repertoire. Once a person has learned to analyze the contingencies to which he/she is exposed and to formulate what are essentially rules, the rules enter into the total contingencies affecting behavior. Nevertheless, I think that verbal behavior and the formulation of rules as descriptions of contingencies

of reinforcement are nothing more than operant behavior. All human behavior is either reflexive (in which case it concerns responses of organs and is of little interest to me) or operant.

11) What kind of empirical evidence, or perceived theoretical deficiency auspiced the formulation of rule-governed behavior?

Skinner: My paper on rule-governed behavior in 1965 was based upon my book *Verbal Behavior* which was not empirical. It was an interpretation of behavior in the light of empirical facts and principles. We behave either because our behavior has been shaped by contingencies of reinforcement or because we have been told, advised or otherwise directed to behave by those whose behavior has already been contingency-shaped. The evolution of the operant control of the human vocal musculature is, I believe, responsible for the human achievements. I do not believe that anything essentially new followed. It simply became easier for the individual to profit from contingencies of selection which already affected other individuals.

[Interviewer's note: Skinner 1966, 1957].

12) What limitations do you perceive in operationalism as a logical rule to build up concepts? Do you think that relations between general operations and systematic effects support the identification of behavioral *processes*? Do you think that the limitations you perceive on operationalism may be pointed out in conditioning theory such as it has been developed by Radical Behaviorism?

Skinner: Logical positivism and operationism arose later than behaviorism. I think we all go back to Ernst Mach, a German physicist, whose book the "Science of Mechanics" influenced me greatly. Although I regard my thesis as an operational analysis, I think it is more than that. I was not reducing the reflex to some other universe of discourse. I was reducing it to some observations. That is equally true of radical behaviorism. Unlike Watson, I molded a science that did not allude to the mind, but also did not allude to the brain. Operant conditioning can be defined without referring to how the body works. It is itself an explanation of why it works that way.

[Interviewer's note: Mach, 1883/1974; Watson, 1913, 1916, 1924/1970].

13) Do you think that your career as a researcher illustrate a *research programme*? Which would be the fundamental components and criteria?

Skinner: I don't regard my work as the effect of a program. I did not plan in advance the way it was to go. I simply followed up one thing after another as the data turned up in my research.

14) Do you think that deprivation states and historical variables are adequately represented in your theoretical formulations?

Skinner: Yes, I think I have given due attention to such things as

deprivation states. See my two very early papers on drive and reflex strength. (Skinner, 1932a, 1932b).

15) Do you think that private events have, before self-tacting, a physical independent functional status?

Skinner: I think the bodily states that we can observe and call emotions and feelings and states of mind all exist before we call them that.

16) According to you, which is the role to be played by mathematization in behavior research and theory?

Skinner: I do not think it is time for mathematization in behavior research and theory until we have data suitable for that purpose. I do not think that mathematical theories are useful.

Skinner: Summary: what I miss in your questions is the whole notion of variation and selection. I think this is important at three levels: Natural selection, operant conditioning, and the evolution of those social environments we call cultures. Only the second of these can be studied experimentally in the laboratory and I think that advantage should be exploited as intensively as possible.

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